of Sciences and to the Cyrus M. Warren Fund of the American Academy of Arts and Sciences for generous assistance which has made this work possible.

[1] Baxter and Starkweather, these PROCEEDINGS, **11,** 231 (1925).

[2] Baxter and Starkweather, *Ibid.*, **10,** 479 (1924).

[3] *Special Pub. No. 40, U. S. Coast and Geodetic Survey,* p. 50 (1919).

[4] Burt, *Trans. Faraday Soc.*, **6,** 19 (1910).

[5] In the report on oxygen the final value 1.42901, was based on the erroneous value for g in the Coolidge Laboratory.

THE NATURE OF LIGHT

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The following views regarding the mode of transmission of light are of so unorthodox a character that I set them down with some reluctance; but it seems to be generally admitted that the paradox of quantum theory cannot be resolved without doing violence to one or more of our cherished common notions. Some of the consequences of the theory that I am going to advance are repugnant to common sense, yet, searching in vain for an alternative and finding no physical fact of optics or of thermodynamics in opposition to the theory, I have come to regard it as a natural and indeed inevitable extension of Einstein's principle of relativity. Let me start with two statements which seem now to be well supported by experiment, although perhaps not entirely demonstrated. They are, however, the postulates upon which the following theory is built.

1. When an emitting atom loses energy of the amount $h\nu$, a particle with the same energy, the momentum $h\nu/c$, and the mass $h\nu/c^2$ may be regarded as traveling from the atom by a definite path until it is absorbed by another atom. The particle travels in a straight line except in the immediate neighborhood of material particles (or perhaps of other light particles) by which it may be deflected or reflected. In each encounter between a particle of light and another mass the two obey the simple laws of conservation of energy, mass and momentum.

2. The phenomenon of interference does not become less marked as the intensity of light becomes feebler, and therefore we may conclude *that the emission of a single light particle from a single atom is subject to the laws of interference.* If an optical system is so arranged that the light from a certain source produces light and dark interference bands on a photographic plate, and the source is now replaced by one in which the individual atoms only rarely emit their particles of light, these particles

will appear on the plate only where the bright bands formerly were, and their number on different parts of the plate will be proportional, on the average, to the former intensity of illumination.

Anyone who accepts these two postulates is obliged to admit that the phenomenon of interference is in some measure independent of the act of transition, and perhaps exists even at times when there is no transfer of light from one atom to another; in other words, that the atom which is capable of emitting light establishes a "field" determining the probability that its particle of light will reach one point or another. This last idea was first advanced by Slater,[1] was somewhat modified by Bohr, Kramers and Slater,[2] and has recently been restated by Slater[3] in its original form. He assumes that an atom capable of emitting a certain frequency possesses a virtual oscillator, of the same frequency, which produces a virtual electromagnetic field; that this field obeys the ordinary electromagnetic laws, and that the magnitude of its virtual Poynting vector determines the probability that a particle of light will reach a given spot. In the same way Swann[4] assumes a field with an energyless Poynting vector so that "we practically constrain the quanta to follow the paths of the Poynting vector." (The suggestion by L. de Broglie[5] that the Poynting vector does not determine the path of the light particle, but only whether the particle may be absorbed at a certain point, is not consistent with our first postulate, since he assumes the conservation laws to be only statistically true.)

Now in addition to an objection mentioned by Slater, there are strong arguments against the idea that the particles of light follow the path of the Poynting vector. In the first place, the ordinary Poynting vector is calculated from the sum of *all* the electric and magnetic fields at a point, while the complete lack of interference of light from distinct sources shows that the field of a single virtual oscillator must by itself produce interference, regardless of the existence of fields from other oscillators. But even if we impose this limitation upon the definition of the virtual Poynting vector, a fatal objection remains. If we admit that a particle of light traverses the distance l between two material objects in the time l/c, and that it cannot move with a velocity greater than c, it must travel in a straight line; but the Poynting vector in an interference field gives a wavy line. Suppose that by means of mirrors we make two portions of the virtual radiation traverse one another at a slight angle, we do not know by experiment that there is absolutely no loss of time in such a process, but certainly there is no such retardation as would be found if the particle of light were forced to follow the tortuous path of the Poynting vector.

Suppose that we do not attempt to decide upon the particular path taken by the particle of light and assume only that the existence of a Poynting vector in a given part of the receiving surface determines whether the particles may reach that part. Here again we are met by contradiction. Figure 1 shows S, a source of light; A and B, two slits; C, a point to which light may go if the slit A alone is open, but which is forbidden if both A and B are open. Now consider that a shutter opens both slits for an instant; if a particle of light were about to pass through A and be deflected in the direction of C, the virtual field passing through B and moving with the velocity of light could not meet the particle until it reached some point such as D. But by this time its direction would have been fully determined and it would proceed to C. This, however, is a forbidden point.

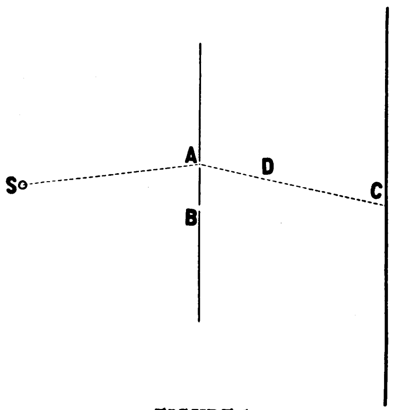

FIGURE 1

So far our analysis has merely shown that no explanation so far proposed explains at the same time the phenomena of quanta and of interference. As we study the simple system described in figure 1, there constantly recurs to us the hardly credible thought that in some manner the atom in the source S can foretell before it emits its quantum of light whether one or both of the slits A and B are going to be open. Although it sounds absurd, this, in a certain sense, is the theory that I am going to propose.

It is generally assumed that a radiating body emits light in every direction, quite regardless of whether there are near or distant objects which may ultimately absorb that light; in other words that it radiates "into space." This assumption has seemed natural and convenient. We know that on a clear night objects radiate energy into what seems empty space, but I am not aware that any exact experiments have been made at different altitudes to eliminate the effect of the atmosphere and to determine whether the emission is that which would be given by Steffan's law. In any case we do not know how much cold matter the universe may contain.

I am going to make the contrary assumption that an atom never emits light except to another atom, and to claim that it is as absurd to think of light emitted by one atom regardless of the existence of a receiving atom as it would be to think of an atom absorbing light without the existence of light to be absorbed. I propose to eliminate the idea of mere emission of light and substitute the idea of *transmission*, or a process of exchange of energy between two definite atoms or molecules. Now, if the process be regarded as a mere exchange, the law of entire equilibrium, which I have recently advanced,[6] requires us to consider the process as a perfectly symmetrical one, so that we can no longer regard one atom as an active agent and the other as an accidental and passive recipient, but both atoms must play coördinate and symmetrical parts in the process of exchange.

I shall not attempt to conceal the conflict between these views and common sense. The light from a distant star is absorbed, let us say, by a molecule of chlorophyl which has recently been produced in a living plant. We say that the light from the star was on its way toward us a thousand years ago. What rapport can there be between the emitting source and this newly made molecule of chlorophyl? Suppose we make this same star the source of light in the apparatus of figure 1. By opening the second slit we prevent a particle of light from reaching the point C. Do we therefore prevent its original emission? If so it would mean that we could, perhaps in a trivial way, but nevertheless in principle, alter the course of past events.

Such an idea is repugnant to all of our notions of causality and temporal sequence; but we must remember that these notions have arisen from the observation of complex processes which are very different from the elementary reversible processes which we are here considering. Unless the result of some actual fact of experiment or observation can be brought against the new view we need not be deterred by this conflict with common notions. Indeed we shall see that there are already some inconsistencies between prevailing physical ideas and that geometry which so admirably interprets the kinematics of relativity.

Admitting that a radioactive substance emits α-rays and β-rays in all directions without regard to their later absorption by other matter, why should we not make the same assumption regarding light? The answer is suggested by the new geometry. Let us consider the four-dimensional manifold of relativity,[7] which for simplicity may be represented in figure 2 by a two-dimensional diagram with one axis of space, OX, and another of time, OT. In the geometry characteristic of this space-time there is a sharp distinction between all lines of the class OX and OX', which Minkowski calls space-like lines, and all time-like lines of the class of OT and OT'. Between these two classes are other lines, the singular lines such as OL and OL', which belong to neither of the other classes and bear no more resemblance to one class than to the other.

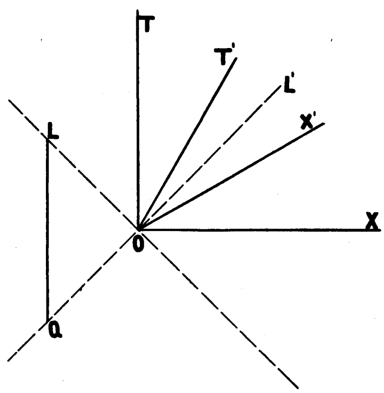

FIGURE 2

The path or locus of a material particle in time and space is always one of the time-like lines, such as OT', and the slope of this line with respect to the chosen time axis OT represents the velocity of the particle. On the other hand, the slope of a space-like line, such as OX', does not even suggest a velocity. Now in spite of the symmetry demanded by the geometry we affiliate the line OL with the time-like lines and say that it represents the velocity of light. As a concession to traditional thought we do violence to our geometry when we call the light process a form of motion at all; and while we shall continue to make this concession, it will be with a realization of the unique character of radiation.

There is another remarkable feature of this geometry which has been insufficiently employed in physics. If two lines such as OT and QL represent the space-time loci of two material particles, then by the methods of measurement that characterize this geometry, the intercepts OL and OQ of singular lines between these loci, are of zero length. This is an idea of which much use has been made in the mathematics but none in the physics of relativity. The proposals which I am making in this paper are tantamount to assuming that such a distance is also zero in a physical sense, and that two atoms whose loci are OT and QL may be said to be in *virtual contact* at any two points such as O and L or O and Q, which are connected by singular lines. When two atoms are in ordinary physical contact we do not inquire how one atom ascertains that the other is in a position to receive energy, nor need we so inquire in the case of virtual contact, even though the time of emission and the time of absorption are said to be separated by thousands of years. Such a statement depends upon an arbitrary choice of a time axis. If in our figure we should take, not OT, but time axes lying nearer and nearer to OL, not only the time elapsing between O and L but also their spatial distance would *approach zero*.

Finally, let us remark that in a pure geometry it would surprise us to find that a true theorem becomes false when the page upon which our figure is drawn is turned upside down. A dissymmetry alien to the pure geometry of relativity has been introduced by our notion of causality. For example, it is the theory of retarded potentials that if, in figure 2, OT represents the locus of an electron and QL the locus of another electron, then the curvature of the line OT at O is determined by the character of the other electron as it is at Q (using the "forward" singular line QO) and not as it is at L (using the "backward" singular line LO). There is, however, no experimental evidence for such an assumption.

In all three of the respects that I have mentioned the theorem that I am proposing, although it still makes concession to our habitual thought, goes a long way toward bringing closer correlation between our physical and our geometrical concepts: it emphasizes the unique character of the singular lines (motion of light), it makes physical use of the theorem of zero distance along singular lines, and it does away with that distinction between past and future which, however useful it may be in other cases, seems to have no significance in a purely reversible process.[6]

Let us represent in figure 3 the path in space-time of two atoms A and B, of which A is capable of losing, and B of gaining the energy $h\nu$. For simplicity we may consider the two atoms relatively at rest and therefore their two loci parallel. Let us now, following the suggestion of Slater's virtual oscillator, draw a helix about the line A to represent in the atom A some sort of periodic motion or change of frequency ν. Let us, however, also draw a helix of the same radius and the same period about the line B. We may speak of the phase of one helix at the point A' and compare it with the phase of the other helix at the point B'. If $A'B'$ is a forward singular line and $A''B''$ is a parallel singular line, then the phase difference between A' and B' will be the same as that between A'' and B'', and may be called simply the phase difference between the two atoms.

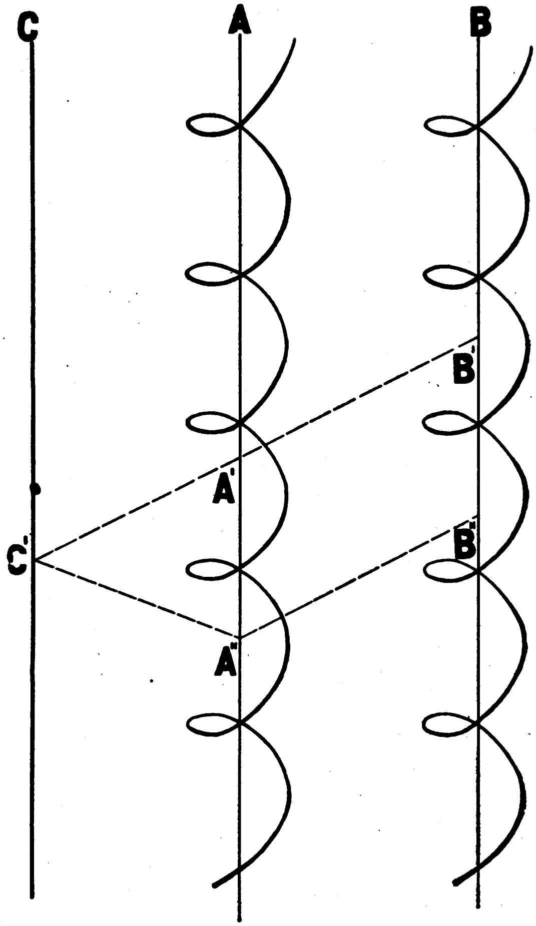

FIGURE 3

In our four-dimensional manifold the two parallel lines A and B determine a perpendicular 3-space, and if we choose a plane in this 3-space the projections of the two helices will be ellipses; but for each helix there is one plane upon which the projection is a circle. The angle between this characteristic plane of A and the characteristic plane of B may be called the relative polarization of the two atoms. Thus we speak not of the phase and polarization of light but of the relative phase and relative polarization of the atoms themselves; and we shall assume that the chance that the atom B accepts radiation from A depends upon these two quantities, and upon the distance between A and B. I must reserve for another communication the demonstration that by proceeding in this simple geometric manner we may derive the quantitative laws of optics. But there are one or two qualitative points which should be mentioned here.

If in addition to our two atoms we introduce a third object which will serve as a mirror, and represent its locus by the line C, then we may draw the singular lines $A''C'$ and $C'B'$ to represent an alternative optical path to $A''B''$. If the two atoms were not in phase with respect to one path, they may be with respect to the other. Or again the phases of A as projected upon B by the two paths may differ by just half a period, in which case, irrespective of the phase of B, the probability of a transfer of energy will be zero (provided that the lengths of the two paths are not materially different).

In addition to these purely geometrical conditions which determine the probability of transfer, it might be supposed that a complete analysis would require knowledge of the atoms from which A obtained its energy and other atoms to which B will ultimately give its energy. But this and other complications may be avoided if we assume that the energy is held by the atom A before the transfer and in the atom B after the transfer for periods which are long compared with the time in which the particles of light may be held by the atoms of the mirror, lenses and other portions of our optical system. If this condition is not met, there are not only mathematical complications but also physical complications, such as anomalous dispersion and lack of coherence; but these are questions which we must discuss in a later paper.

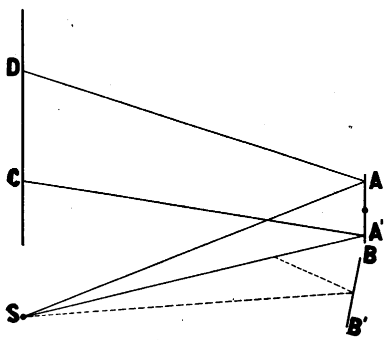

FIGURE 4

Finally, it is legitimate to inquire whether a theory which claims to resolve a serious paradox and also to bring our physical concepts more nearly into accord with the geometry of relativity, can also make a new experimental prediction. In fact there is a crucial experiment which does not seem absolutely beyond the reach of our present experimental technique. Figure 3 shows a source of light, S, and two mirrors, AA' and BB', so adjusted as to produce an interference pattern upon the plate CD. Now let us suppose that the mirror AA' is so narrow that its width is only one-half the distance between the adjoining dark and light bands C and D, and let it be suspended in such manner as to permit rotation in the plane of the diagram. According to the classical theory, and also according to the views set forth by Slater and by Swann, the Poynting vector being the same at A and A', the pressure of light will be uniform over this mirror. According to the new theory, particles of light will pass over the path SAD where D is in the bright band, but will not pass over the path $SA'C$ if C is in the dark band. Therefore, the light pressure upon this mirror will all be on the side A and a torsion of the mirror will result.

[1] Slater, *Nature*, **113**, 307 (1924).
[2] Bohr, Kramers and Slater, *Phil. Mag.*, **47**, 785 (1924).
[3] Slater, *Nature*, **116**, 127 (1925).
[4] Swann, *Science*, **61**, 425 (1925).
[5] L. de Broglie, *Phil. Mag.*, **47**, 446 (1924).
[6] Lewis, these PROCEEDINGS, **11**, 179 (1925); **11**, 422 (1925).
[7] Wilson and Lewis, *Proc. Amer. Acad.*, **48**, 389 (1912).

THE RÔLE OF THE FARADAY CYLINDER IN THE MEASUREMENT OF ELECTRON CURRENTS

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Communicated December 11, 1925

Despite knowledge of the magnitude of reflection and secondary emission of electrons, Faraday cylinders have customarily been regarded as complete absorbers of electrons. For example Lehmann and Osgood[2] recently have brought forward evidence indicating that electrons passing through small apertures are not of homogeneous velocities—this conclusion resting on the assumption that the Faraday cylinder used in their experiments absorbed at least 99% of the impinging electron stream. An exception to this usual regard of the matter is recorded in some experiments of Professor J. T. Tate[3] who found that the most efficient Faraday cylinder he devised had an absorption coefficient of about 0.95 for relatively slow velocity electrons. In view of the very important relation of Lehmann and Osgood's conclusions to much other experimental work and, indeed, because of the uncertain yet crucial status of the Faraday cylinder itself, this preliminary experimental investigation was carried out.

Electrons were accelerated through a distance of 4 cms. from a tungsten filament to a plane anode through which extended a tube 2 mm. in diameter and 16 mm. in length, the tube end nearer the filament being in the anode plane. The electrons emerging from the tube impinged on a Faraday cylinder 2.2 cms. in diameter through an opening 1 cm. in diameter at a distance of 1 mm. from the end of the tube. The distance of the closed end of the Faraday cylinder—it's effective length—was made variable by a stopcock swivel arrangement. Since the absorbing power of the Fara-